The classical problem of the functions of DNA methylation in vertebrates is discussed by Adrian Bird. In the vertebrates almost all regions of the genome are subject to methylation, while in non-vertebrates, which include nearly all animal species, most of the genome appears free of methylation at all times, and the methylation that does occur in these genomes is confined to a small fraction of the nuclear DNA. To explain this striking difference Bird suggests that DNA methylation acquired a new function at the start of the vertebrate lineage, which made possible the increase in the number of usable genes necessary for the dramatic progress in vertebrate evolution.

Telomeres and telomerase are a subject of great interest at present, since telomeres, the structures at the ends of eukaryotic chromosomes, serve the two vital functions of maintaining the length of the chromosomes in the face of the inability of DNA polymerase to replicate linear DNA ends completely, and of distinguishing natural chromosome ends from double-stranded breaks in the DNA, which must be rapidly repaired. Most eukaryotes have a short, tandemly repeated, evolutionarily conserved sequence on their chromosome ends, and these arrays, shortened at replication as indicated above, are extended by a specific reverse transcriptase, which carries its own internal RNA template. *Drosophila*, however, maintains chromosome length by tip-specific transposition of a small set of retrotransposons (James M. Mason & Harald Biessmann (1995) The unusual telomeres of *Drosophila*. TIG Vol. 11, No. 2, 58–62). Several papers in the book under review present experimental evidence on eukaryote telomeres and telomerase (pp. 707–746), but *Drosophila* was not included. Another article which probably everyone will want to read is 'The replicon: thirty years later' by Francois Jacob, whose beautifully composed English and impeccable logic was a great pleasure to read when I first studied bacterial genetics.

This might just be the book to be marooned with on the desert island reserved for Desert Island Disks, and if you are allowed a second book and your knowledge of molecular biology needs upgrading, there is the Encyclopedia of Molecular Biology edited by John Kendrew assisted by 11 stars (Blackwell 1994).

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Lords of the Fly by R. E. KOHLER. University of Chicago Press. 1984. 321 pages. Price £14.25. ISBN 0 226 45063 5

This book is by a science historian who adopts an unconventional approach: he attempts to write 'a material, cultural and social history of scientists at work'; specifically, the story of the research community using *Drosophila*. However, the author has a thesis that 'scientists work neither out of pure curiosity nor to win rewards... but rather to gain the continuing privilege of working under ideal conditions'. Why do science historians have to have theses to live by even when their own studies refute such simple-minded categorizations? I remember Kitty Brehme telling me that she went into Bridges' darkened hut at Cold Spring Harbor when he was drawing the details of salivary gland band patterns and heard him repeat the mantra, 'Christ, what a life for a man!' Ideal conditions/no curiosity, I ask you? But despite this bias the book is a success, and all new (and recent) entrants to the *Drosophila* fellowship should read it.

This is not a history of *Drosophila* as an experimental organism for it says little about the physiological studies of Loeb, Northrop, Guyenot and others whose successors still pursue some of the issues raised around the turn of the century. It is about how *Drosophila* was developed as a tool for genetical research: in the first place, it is the story of the Columbia Fly Room established by the embryologist T. H. Morgan with his eye on the exploitation of mutations for the understanding of evolution and development. But this is not how things worked out during 1909–10, after Sturtevant and Bridges decided to classify the mutations they found not according to affected organs but by chromosomal groups. This started the great flurry of activity around chromosome mapping and by 1914, thanks to Muller and Bridges, to the construction of multiple marker stocks and balancer chromosomes, which became the ever improving tools of the trade. At this point, domesticated *Drosophila* had a capital value; namely, the intellectual and practical investment in these specialized stocks and technology. *Drosophila* effectively took over the laboratory (Kohler calls its the 'breeder reactor') and determined its ethos; or in E. P. Thomson's phrase which he uses, its 'moral economy'.

That 'moral economy' owed something, no doubt, to the fact that *Drosophila* was of no commercial value, but very much more to the gross overcrowding of the Fly Room. This was the working space for Morgan and the Carnegie Trust supported Sturtevant, Bridges and Muller (and that Trust merits a vote of thanks from all Drosophilists) and a succession of graduate students and visitors. It was a small (16 × 25 ft), exciting world of great activity and shared experiences. As Jack Schultz said to me, 'there were no patents on ideas, they were bandied about freely'; publication was the mark of recognition. There is a tendency to think that these first years of *Drosophila* genetics reflected the true, impersonal pattern of science, with a capital S; but it would be fairer to say that this highly competitive group of workers recognized rules that were to their mutual advantage; and, essentially, this was to share everything – information, technology, fly stocks and ideas, without reservation. So *Drosophila* rapidly became the dominant organism in genetic research, carrying this ethos with it. And that was a great tradition for us, accepted without

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question! So much for the fly; what about the Lords?

Morgan was a tenured Professor of Zoology at Columbia. As the only Faculty member in the fly group, he alone could take students. But he was casual about this, generally passing them to Sturtevant and Bridges who taught them techniques and, to some degree, organized their work. Morgan himself mucked-in (and that is the right phrase for the Fly Room was an overcrowded tip), taking his share of scoring other people's flies, continually informing the group about what was going on, to the extent of reading out his mail as he opened it. There is much to be learnt from this uninhibited and cooperative laboratory set-up which Morgan led; but I am afraid the Health and Safety Executive would ban any place like it today! We have lost a lot of human spontaneity the more we have become managed by society. Kohler's text illustrates this very well.

While Morgan was 'the Boss', Sturtevant and Bridges were 'his boys', and he kept them in the subordinate position through his manipulation and control of the Carnegie funds. Sturtevant was the intellectual of the pair and Bridges the brilliant, and complementary, technician. Sturtevant obviously understood the significance of the work being done and kept abreast of researches on plants, especially the experiments of plant breeders, but the handsome Bridges concentrated on the immediate work in hand and on fulfilling his belief in free love and communism – in that order. So there is plenty of interesting anecdotal information about the relationships between these three key players. There's nothing new about lab. gossip or its, apparently eternal, interest: Kohler illustrates its role in keeping the lab. together.

It could not last since organizations, like individuals, have a life cycle (little studied by historians) and while his boys remained almost subservient to the Boss, newcomers like Muller and Metz found the 'moral economy' restrictive and moved away. Still, 'no trade secrets, no monopolies, no poaching, no ambushes' remained the practical rules for establishing trust and harmony among the fly people! And this was consolidated on the publication by Demerec and Bridges of the *Drosophila* Informative Service (1934) which became the public means for transmission of craft procedures, of stock lists and of news of new mutants; as it does to this day. Before that *Drosophila* work had survived the disruptive move of Morgan *et al.* to Cal Tech (1929); but it had also received a great boost from Painter's (1933) discovery of the polytene salivary gland chromosomes. Recombinant maps could now be related to chromosome structures: Bridges formulated the programme which was to be his last contribution to *Drosophila* genetics as relating one cross band to one locus. Thus recombination, mapping and chromosome studies were given a new lease of life.

Since the stocks were designed for mapping, they proved unsuitable for Sturtevant's and Schultz's early attempts to use the fly to study development (gynanders, gene dosage effects, eye colour, gene interactions etc.). Surprisingly, Kohler does not note that the group's exclusive attention to adult features precluded them from looking at embryonic and larval lethals which, a generation later, were to make *Drosophila* the organism of choice for development studies. But he is very instructive, and accurate, in his discussion of the quandary with which the synthesized fly confronted those who questioned the thesis that maps equalled genetics. The break-through which Beadle and Ephrussi made by using the embyologist's transplantation technique (of different eye colour imaginal discs) proved to be a false steer since they were studying the biochemical syntheses of eye pigments, not development. But it did bring biochemistry into *Drosophila* studies; and with the 'bitch the other guy if you can' attitude of the biochemists!

Initial attempts to relate genes to evolution using species crosses generally failed as a result of inviability, and ended with the usual, laborious mapping of these species. *D. melanogaster* chosen for its physiological adaptability was also useless, and it was only when Sturtevant and Dobzhansky found the undomesticated *D. pseudoobscura* and used its natural genetic diversity to adapt to geographical and climatic difference that it became possible to trace the spread of adaptive mutations through populations, using salivary gland chromosome analyses. This work was summarized in Dobzhansky's '*Genetics and the Origin of Species*', and marked the second break in the 30 yr mapping tradition.

Kohler's careful history covers almost precisely the same material as Sturtevant and Beadle's *An Introduction of Genetics* (1939) but it provides the inside story of what was really going on. So not only should Drosophilists read it for its intrinsic interest, but so should those who sit on grant giving committees for here they will learn how science really functions at the level of individuals (Lords, do you think; or just historically lucky?). I hope the success of this book will encourage the author to sort out the post-war activities of *D. melanogaster*, in a very different cultural environment.

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Molecular Ecology and Evolution: Approaches and Applications. Edited by B. Schierwater, B. Streit, G. P. Wagner and R. DeSalle. Birkhäuser Verlag, Basel. 1994. 640 pages. Hard cover. Price £98. US$ 165.00. ISBN 3 7643 2942 4 (Basel). ISBN 0 8176 2942 4 (Boston).

Studies in ecology and evolution have gained great impetus from the new techniques of molecular biology, so that many previously intractable problems are

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